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FRIDAY, APRIL 27, 1900.

LIQUID HYDROGEN.*

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FROM the year 1878, when the experiments of Cailletet and Pictet were attracting the attention of the scientific world, it became a common habit in textbooks to speak of all the permanent gases, without any qualification, as having been liquefied, whereas these experimenters, by the production of an instantaneous mist in a glass tube of small bore, or a transitory liquid jet in a gas expanding under high compression into air, had only adduced evidence that sooner or later the static liquid form of all the known gases would be attained. Neither Pictet nor Cailletet in their experiments ever succeeded in collecting any of the permanent gases in that liquid form for scientific examination. Yet we meet continually in scientific literature with expressions which lead one to believe that they did. For instance, the following extract from the 'Proceedings' of the Royal Society, 1878, illustrates this point very well: "This award (Davy Medal) is made to these distinguished men (Cailletet and Pictet) for having independently and contemporaneously liquefied the whole of the gases hitherto called permanent." Many other quotations of the same kind may be made. As a matter of fact six years elapsed, during which active investigation in this department was being prose-

* Lecture before the Royal Institution of Great Britain.

cuted, before Wroblewski and Olszewski succeeded in obtaining oxygen as a static liquid, and to collect liquid hydrogen, which is a much more difficult problem, has taken just twenty years from the date of the Pictet and Cailletet experiments.

Wroblewski made the first conclusive experiment on the liquefaction of hydrogen in January, 1884. He found that the gas cooled in a capillary glass tube to the boiling point of oxygen, and expanded quickly from 100 to 1 atmosphere, showed the same appearance of sudden ebullition lasting for a fraction of a second, as Cailletet had seen in his early oxygen experiments. No sooner had the announcement been made, than Olszewski confirmed the result by expanding hydrogen from 190 atmospheres, previously cooled to the temperature given by liquid oxygen and nitrogen evaporating under diminished pressure. Olszewski, however, declared in 1884 that he saw colorless drops, and by partial expansion to 40 atmospheres, the liquid hydrogen was seen by him running down the tube. Wroblewski could not confirm Olszewski's results, his hydrogen being always obtained in the form of what he called a 'liquide dynamique,' or the appearance of an instantaneous froth. Olszewski himself seven years later repeated his experiments of 1884 on a larger scale, confirming Wroblewski's results, thereby proving that the so-called liquid hydrogen of the earlier experiments must have been due to some impurity. The following extract from Wroblewski's paper states very clearly the results of his work on Hydrogen:

"L'hydrogène soumis à la pression de 180 atm. jusqu'à 190 atm., refroidi par l'azote bouillant dans la vide (à la température de sa solidification) et détendu brusquement sous la pression atmosphérique présente une mousse bien visible. De la couleur grise de cette mousse, où l'œil ne peut distinguer des gouttelettes incolores, on ne peut pas encore deviner quelle apparence aurait

l'hydrogène à l'état de liquide statique et l'on est encore moins autorisé à préciser s'il a ou non une apparence métallique. J'ai pu placer dans cette mousse ma pile thermo-électrique, et j'ai obtenu suivant les pressions employées des températures de -208° jusqu'à -211° C. Je ne peux pas encore dire dans quelle relation se trouvent ces nombres avec la température réelle de la mousse ou avec la température d'ébullition de l'hydrogène sous la pression atmosphérique, puisque je n'ai pas encore la certitude que la faible durée de ce phénomène ait permis à la pile de se refroidir complètement. Néanmoins, je crois aujourd'hui de mon devoir de publier ces résultats, afin de préciser l'état actuel de la question de la liquéfaction de l'hydrogène."*

It is well to note that the lowest thermo-electric temperature recorded by Wroblewski during the adiabatic expansion of the hydrogen (namely, -211°) is really equivalent to a much lower temperature on the gas-thermometer scale. The most probable value is -230° , and this must be regarded as the highest temperature of the liquid state, or the critical point of hydrogen, according to his observations. In a posthumous paper of Wroblewski's on 'The Compression of Hydrogen,' published in 1889, an account appears of further attempts which he had made to liquefy hydrogen. The gas compressed to 110 atmospheres, was cooled by means of liquid nitrogen under exhaustion to -213.8° . By suddenly reducing the pressure, as low a temperature as -223° on his scale was recorded, but without any signs of liquefaction. This expansion gives a theoretical temperature of about 15° absolute in the gas particles. The above methods having failed to produce static hydrogen, Wroblewski suggested that the result might be attained by the use of hydrogen gas as a cooling agent. From this time until his death in the year

* Compt. Rend., 1885, 100, 981.

1888, Wroblewski devoted his time to a laborious research on the isothermals of hydrogen at low temperatures. The data thus arrived at enabled him, by the use of Van der Waal's formulæ, to calculate the critical constants, and also the boiling point of liquid hydrogen.

Olszewski returned to the subject in 1891, repeating and correcting his old experiments of 1884, which Wroblewski had failed to confirm, using now a glass tube 7 mm. in diameter instead of one of 2 mm. as in the early trials. He says: "On repeating my former experiments, I had no hope of obtaining a lower temperature by means of any cooling agent, but I hoped that the expansion of hydrogen would be more efficacious, on account of the larger scale on which the experiments were made." The results of these experiments Olszewski describes as follows: "The phenomenon of hydrogen ebullition, which was then observed, was much more marked and much longer than during my former investigations in the same direction. But even then I could not perceive any meniscus of liquid hydrogen." Further, "The reason for which it has not hitherto been possible to liquefy hydrogen in a static state, is that there exists no gas having a density between those of hydrogen and of nitrogen, and which might be for instance 7-10 ($H = 1$). Such a gas could be liquefied by means of liquid oxygen or air as cooling agent, and be afterwards used as a frigorific menstruum in the liquefaction of hydrogen."

Professor Olszewski, in 1895, determined the temperature reached in the momentary adiabatic expansion of hydrogen at low temperatures, just as Wroblewski had done in 1885, only he employed a platinum-resistance thermometer instead of a thermojunction. For this purpose he used a small steel bottle of 20 or 30 cc. capacity, containing a platinum-resistance thermometer; in this way the temperatures registered

were regarded as those of the critical and boiling points of liquid hydrogen, a substance which could not be seen under the circumstances and was only assumed to exist for a second or two during the expansion of the gaseous hydrogen in the small steel bottle.

The results arrived at by Wroblewski and Olszewski are given in the following table, and it will be shown later on that Wroblewski's constants are nearest the truth.

	Wroblewski, 1885.	Olszewski, 1895.
Critical temperature.....	— 240°	— 234°
Boiling point.....	— 250°	— 243°
Critical pressure.....	13 atm.	20 atm.

The accuracy of Wroblewski's deductions regarding the chief constants of liquid hydrogen following from a study of the isothermals of the gas is a signal triumph for the theory of Van der Waals and a monument to the genius of the Cracow physicist. From these results we may safely infer that supposing a gas is hereafter discovered in small quantity four times more volatile than liquid hydrogen, having a boiling point of about 5° absolute, and therefore incapable of direct liquefaction by the use of liquid hydrogen, yet by a study of its isothermals we shall succeed in finding out its most important liquid constants, although the isolation of the real liquid may for the time be impossible.

In a paper published in the *Philosophical Magazine*, September, 1884, 'On the Liquefaction of Oxygen and the Critical Volumes of Fluids,' the suggestion was made that the critical pressure of hydrogen was wrong, and that instead of being 99 atmospheres (as deduced by Sarrau from Amagat's isothermals) the gas had probably an abnormally low value for this constant. This view was substantially confirmed by Wroblewski finding the critical pressure of 13.3 atmospheres, or about one-fourth of that of oxygen. The *Chemical News*, Septem-

ber 7, 1894, contains an account of the stage the author's hydrogen experiments had reached at that date. The object was to collect liquid hydrogen at its boiling point, in an open vacuum vessel, which is a much more difficult problem than seeing it in a glass tube under pressure and at a higher temperature. In order to raise the critical point of hydrogen to about -210° , from 2 to 5 per cent. of nitrogen or air was mixed with it. *This is simply making an artificial gas containing a large proportion of hydrogen which is capable of liquefaction by the use of liquid air.* The results are summed up in the following extract from the paper: "One thing can, however, be proved by the use of the gaseous mixture of hydrogen and nitrogen, namely that by subjecting it to a high compression at a temperature of -200° and expanding the resulting liquid into air, a much lower temperature than anything that has been recorded up to the present time can be reached. This is proved by the fact that such a mixed gas gives, under the conditions, a paste or jelly of solid nitrogen, evidently giving off hydrogen, because the gas coming off burns fiercely. Even when hydrogen containing only some 2 to 5 per cent. of air is similarly treated, the result is a white solid matter (solid air) along with a clear liquid of low density, which is so exceedingly volatile that no known device for collecting it has been successful." This was in all probability the first liquid hydrogen obtained, and the method is applicable to other difficultly liquefiable gases.

Continuing the investigations during the winter of 1894, and the greater part of 1895, the author read a paper before the Chemical Society in December of that year entitled, 'The Liquefaction of Air and Research at Low Temperatures,'* in which occasion was taken to describe for the first

time the mode of production and use of a Liquid Hydrogen Jet. At the same meeting Professor William Ramsay made an announcement of a sensational character, which amounted to stating that my hydrogen results had been not only anticipated but bettered. The statement made to the Society by Professor Ramsay, reads as follows: "*Professor Olszewski had succeeded in liquefying hydrogen, and from unpublished information received from Cracow, he (Ramsay) was able to state that a fair amount of liquid had been obtained, not as a froth, but in a state of quiet ebullition, by surrounding a tube containing compressed hydrogen by another tube also containing compressed hydrogen at the temperature of oxygen boiling at a very low pressure. On allowing the hydrogen in the middle jacket suddenly to expand, the hydrogen in the innermost tube liquefied, and was seen to have a meniscus. Its critical point and its boiling point, under atmospheric pressure, were determined by means of a resistance thermometer.*" *

This announcement of Professor Ramsay's had from its very specific and detailed experimental character the merit of the appearance of being genuine, although it was never substantiated, either by the production of the Cracow document, or by any subsequent publication of such important results by Professor Olszewski himself. My observation at the time on Professor Ramsay's communication was that quotations had been made in my paper from the most recent publications of Professor Olszewski in which he made no mention of getting 'Static Hydrogen or of seeing a meniscus' or of getting what Professor Ramsay alleged 'a fair amount of liquid, not as a froth, but in state of quiet ebullition.' To achieve such a result would require a very different scale of experiment from anything Professor Olszewski had so far described. Naturally an early corroboration of the startling statement

* 'Proceedings' of the Chemical Society, No. 158, 1895.

* 'Proceedings' of the Chemical Society, No. 195, 1897-1898.

made by Professor Ramsay as to this alleged anticipation was expected, but strange to say Professor Olszewski published no confirmations of the experiments detailed by Professor Ramsay in scientific journals of date immediately preceding my paper or during the following years 1896, 1897 or up to May, 1898. The moment the announcement was made by me to the Royal Society in May, 1898 that, after years of labor, hydrogen had at last been obtained as a static liquid, Professor Ramsay repeated the story to the Royal Society that Olszewski had anticipated my results (basing the assertion solely on the contents of the old letter received some two and a half years before), in spite of the fact that during the interval he, Professor Ramsay, must have known that Professor Olszewski had never corroborated in any publication either the form of the experiments he had so minutely described or the results which were said to follow. Challenged by me at the Royal Society Meeting on May 12, 1898, to produce Olszewski's letter of 1895, he did not do so, but at the next meeting of the Society, Professor Ramsay read a letter he had received during the interval from Professor Olszewski, denying that he had ever stated that he had succeeded in producing static liquid hydrogen. This oral communication of the contents of the new Olszewski letter (of which it is to be regretted there is no record in the published proceedings of the Royal Society) is the only kind of retraction Professor Ramsay has thought fit to make of his published misstatements of facts. No satisfactory explanation has yet been given by Professor Ramsay of his twice repeated categorical statements made before scientific bodies of the results of experiments which, in fact, had never been made by their alleged author. The publicity that has been given to this controversy makes it imperative that the matter should not be passed over, but once for all recorded.

The report of a Friday Evening Discourse on 'New Researches on Liquid Air,'* contains a drawing of the apparatus employed for the production of a jet of hydrogen containing visible liquid. This is reproduced

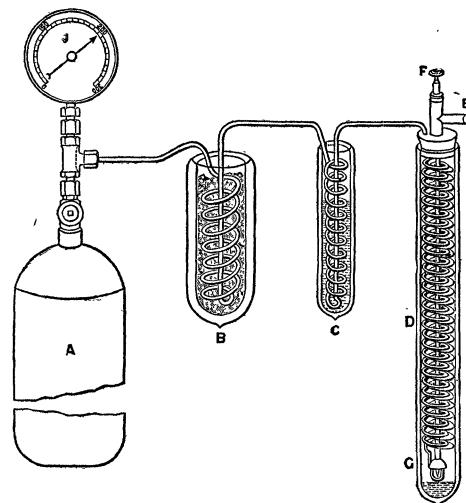


FIG. 1.

in Fig. 1. *A* represents one of the hydrogen cylinders; *B* and *C*, vacuum vessels containing carbolic acid under exhaustion and liquid air respectively; *D* is the coil, *G* the pin-hole nozzle, and *F* the valve. By means of this hydrogen jet, liquid air can be quickly transformed into a hard solid. It was shown that such a jet could be used to cool bodies below the temperature that it is possible to reach by the use of liquid air, but all attempts to collect the liquid hydrogen from the jet in vacuum vessels failed. No other investigator improved on my results,† or has indeed touched the subject during the last three years. The type of apparatus used in these experiments worked well, so it was resolved to construct a much larger liquid-air plant, and to combine with it circuits and arrangements for the liquefaction of hy-

* 'Proceedings' of the Royal Institution, 1896.

† 'Proceedings of the Chemical Society' (No. 158), 1895.

drogen. This apparatus took a year to build, and many months have been occupied in the testing and preliminary trials. The many failures and defects need not be detailed.

On May 10, 1898, starting with hydrogen cooled to -205° , and under a pressure of 180 atmospheres, escaping continuously from the nozzle of a coil of pipe at the rate of about 10 to 15 cubic feet per minute, in a vacuum vessel doubly silvered and of special construction, all surrounded with a space kept below -200° , liquid hydrogen commenced to drop from this vacuum vessel into another doubly isolated by being surrounded with a third vacuum vessel. In about five minutes, 20 c.c. of liquid hydrogen were collected, when the hydrogen jet froze up, from the accumulation of air in the pipes frozen out from the impure hydrogen. The yield of liquid was about one per cent. of the gas. The hydrogen in the liquid condition is clear and colorless, showing no absorption spectrum, and the meniscus is as well defined as in the case of liquid air. The liquid must have a relatively high refractive index and dispersion, and the density appears at first sight to be in excess of the theoretical density, namely 0.18 to 0.12, which we deduce respectively from the atomic volume of organic compounds, and the limiting density found by Amagat for hydrogen gas under infinite compression. A preliminary attempt, however, to weigh a small glass bulb in the liquid made the density only about 0.08, or half the theoretical. My old experiments on the density of hydrogen in palladium gave a value for the combined element of 0.62. Not having arrangements at hand to determine the boiling point other than a thermo-junction which gave entirely fallacious results, experiments were made to prove the excessively low temperature of the boiling fluid. In the first place if a long piece of glass tubing, sealed at one end and open to the

air at the other, is cooled by immersing the closed end in the liquid hydrogen, the tube immediately fills where it is cooled with solid air. A small glass tube filled with liquid oxygen when cooled in liquid hydrogen is transformed into a bluish white solid. This is a proof that the boiling point of hydrogen is much lower than any temperature previously reached by the use of liquid nitrogen evaporating *in vacuo*, seeing oxygen always remains liquid under such conditions. A first trial of putting liquid hydrogen under exhaustion gave no appearance of transition into the solid state. When the vacuum tube containing liquid hydrogen is immersed in liquid air so that the external wall of the vacuum vessel is maintained at about -190° , the hydrogen is found to evaporate at a rate not far removed from that of liquid air from a similar vacuum vessel under the ordinary conditions of temperature. This leads me to the conclusion that, with proper isolation, it will be possible to manipulate liquid hydrogen as easily as liquid air.

The boiling point of liquid hydrogen at atmospheric pressure in the first instance was determined by a *platinum-resistance thermometer*. This was constructed of pure metal and had a resistance of 5.3 ohms at 0° C., which fell to about 0.1 ohm when the thermometer was immersed in liquid hydrogen. The reduction of this resistance to normal air thermometer degrees gave the boiling points -238.2° and -238.9° respectively by two extrapolation methods, and -237° by a Dickson formula.* The boiling point of the liquid seems therefore to be -238° C. or 35° absolute, and is thus about 5° higher than that obtained by Olszewski by the adiabatic expansion of the compressed gas, and about 8° higher than that deduced by Wroblewski from Van der Waal's equation. From these results it may be inferred that the critical point of hydrogen is about 50° absolute,

* See *Phil. Mag.*, 45, 525, 1898.

and that the critical pressure will probably not exceed 15 atmospheres.

If we assume the resistance reduced to zero, then the temperature registered by the electric thermometer ought to be -244° C. At the boiling point of hydrogen, registered by the electric-resistance thermometer, if the law correlating resistance and temperature can be pressed to its limits, a lowering of the boiling point of hydrogen by 5° or 6° C., would therefore produce a condition of affairs in which the platinum would have no resistance, or would become a perfect conductor. Now we have every reason to believe that hydrogen, like other liquids, will boil at a lower temperature the lower the pressure under which it is volatilized. The question arises, how much lowering of the temperature can we practically anticipate? For this purpose we have the *boiling point given by the hydrogen gas thermometer*, and critical data available, from which we can calculate an approximate vapor pressure formula, accepting 22° absolute as about the boiling point, 33° absolute as the critical temperature, and 15.4 atmospheres as the critical pressure; then, as a first approximation—

$$\log. p = 6.410 - \frac{77.62}{T} \text{ mm. . . . (1)}$$

If, instead of using the critical pressure in the calculation, we assume the molecular latent heat of hydrogen to be proportional to the absolute boiling point, then, from a comparison with an expression of the same kind, which gives accurate results for oxygen tensions below one atmosphere, we can derive another expression for hydrogen vapor pressures, which ought to be applicable to boiling points under reduced pressure.

The resulting formula is—

$$\log. p = 7.0808 - \frac{88}{T} \text{ mm. . . . (2)}$$

Now formula (1) gives a boiling point of 14.2° absolute under a pressure of 25 mm.,

whereas the second equation (2) gives for the same pressure 15.4° absolute. As the absolute boiling point under atmospheric pressure is about 22° , both expressions lead to the conclusion that ebullition under 25 mm. pressure ought to reduce the boiling point some 7° C. For some time experiments have been in progress with the object of determining the temperature of hydrogen boiling under about 25 mm. pressure, by the use of the platinum thermometer; but the difficulties encountered have been great, and repeated failures very exasperating. The troubles arise from the conduction of heat by the leads; the small latent heat of hydrogen, volume for volume, as compared with liquid air; the inefficiency of heat isolation; and the strain on the thermometer, brought about by solid air freezing on it and distorting the coil of wire. In many experiments, the result has been that all the liquid hydrogen has evaporated

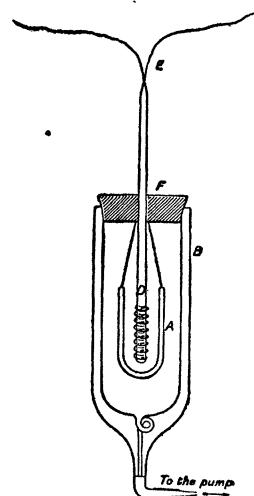


FIG. 2.

before the pressure was reduced to 25 mm., or the thermometer was left imperfectly covered. The apparatus employed will be understood from Fig. 2. The liquid hydrogen collected in the vacuum vessel *A* was suspended in a larger vessel of the same kind, *B*, which is so constructed that a

spiral tube joins the inner and outer test-tubes of which *B* is made, thereby making an opening into the interior at *C*. The resistance thermometer *D* and leads *E* pass through a rubber cork, *F*, and the exhaustion takes place through *C*. In this way the cold vapors are drawn over the outside of the hydrogen vacuum vessel, and this helps to isolate the liquid from the convective currents of gas. To effect proper isolation, the whole apparatus ought to be immersed in liquid air under exhaustion. Arrangements of this kind add to the complication,

reducing the pressure, the resistance diminished to 0.114 ohm, and kept steady for some time. The lowest reading of resistance was 0.112 ohm. This value corresponds to -2.391° C., or only one degree lower on its own scale, than the boiling point at atmospheric pressure, whereas the temperature ought to have been reduced at least 5°, under the assumed exhaustion, according to the gas thermometer scale. The position of the observation on the curve of the relation of temperature and resistance for No. 7 thermometer is shown

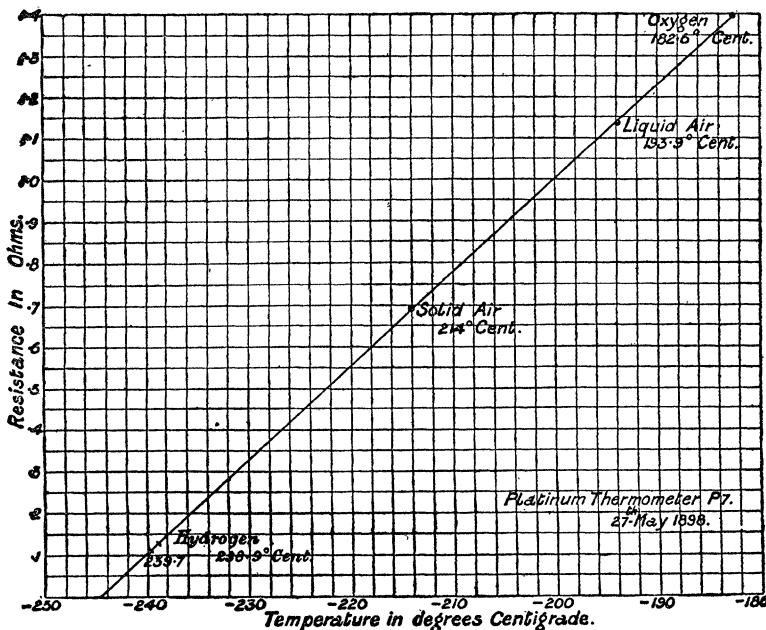


FIG. 3.

so in the first instance the liquid was used as described. The liquid hydrogen evaporated quietly and steadily under a diminished pressure of about 25 mm. Naturally the liquid does not last long, so the resistance has to be taken quickly. Just before the reduction of pressure began, the resistance of the thermometer was 0.131 ohm. This result compares favorably with the former observation on the boiling point, which gave a resistance of 0.129 ohm. On

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on the accompanying diagram (Fig. 3). As a matter of fact, however, this platinum thermometer was, when placed in liquid hydrogen, cooled at starting below its own temperature of perfect conductivity, so that no exhaustion was needed to bring it to this point. The question arises then as to what is the explanation of this result? Has the platinum resistance thermometer arrived at a limiting resistance about the boiling point of hydrogen, so that at a lower

temperature its changes in resistance become relatively small—the curve having become practically asymptotic to the axis of temperature? That is the most probable supposition, and it further explains the fact that the temperature of boiling hydrogen obtained by the linear extrapolation of the resistance temperature results in values that are not low enough.

As the molecular latent heats of liquids are proportional to their absolute boiling points, the latent heat of liquid hydrogen will be about two-fifths that of liquid oxygen. It will be shown later, however, that we can reach from 14° to 15° absolute by the evaporation of liquid hydrogen under exhaustion. From analogy, it is probable that the practicable lowering of temperature to be obtained by evaporating liquid hydrogen under pressures of a few mm., cannot amount to more than 10° to 12° C., and it may be said with certainty that, assuming the boiling point 35° absolute to be correct, no means are at present known for approaching nearer than 20° to 25° to the absolute zero of temperature. The true boiling point is in reality about -252° C., in terms of the *gas-thermometer scale*, and the latent heat of the liquid is, therefore, about two-ninths that of an equal volume of oxygen, or one-fourth that of liquid nitrogen. The platinum-resistance thermometer had a zero point of -263.2 platinum degrees, and when immersed in boiling liquid hydrogen, indicated a temperature of -256.8° on the same scale, or 6.4 platinum degrees from the point at which the metal would theoretically become a perfect conductor. The effect of cooling platinum from the boiling point of liquid oxygen to that of liquid hydrogen is to diminish its resistance to one-eleventh.

The difficulties in liquefying hydrogen caused by the presence of air in the gas have been referred to,* and later experi-

ments had for their object the removal of this fruitful source of trouble. This is by no means an easy task, as quantities amounting to only a fraction of one per cent. accumulate in the solid state, and eventually choke the nozzle of the apparatus, necessitating the abandonment of the operation.

Later experiments enabled me to procure a larger supply of liquid hydrogen with which the determination of certain physical constants has been continued. The first observations made with a pure platinum-resistance thermometer had given -238° as the boiling point. A new thermometer, constructed of platinum from a different source, gave practically the same value. As these results might be affected by some constant error, the determination was checked by employing a thermometer constructed from an alloy of rhodium and platinum, containing 10 per cent. of the former. Alloys had been shown by Professor Fleming and the author to differ from pure metals in showing no sign of becoming perfect conductors at the absolute zero of temperature, and a study of the rhodium-platinum alloy had shown that the change in conductivity produced by cooling from 0° to the boiling point of liquid air is regular and may be represented by a straight line. As determined by the rhodium-platinum thermometer, the boiling point of hydrogen was found to be -246° or some 8° lower than the platinum thermometer gave. Two ways of explaining the discrepancy between the two values suggested themselves. Pure platinum, although its resistance may be represented by a straight line almost down to the solidifying point of air, shows signs of a departure from regularity at about this point, and the curve may become asymptotic at lower temperatures. On the other hand, the resistance of the rhodium-platinum alloy diminishes less rapidly at these lower temperatures and is much higher than that of pure platinum under

* 'Proceedings,' 1898, 14, 130.

similar conditions. It follows that its resistance curve, in all probability, deviates less from a straight line than is the case with platinum. Either cause would explain the differences observed, but the lower boiling point (-246° or 27° absolute) seemed to be the more probable as it agreed very fairly with the value for the boiling point calculated by the author from Wroblewski's results. As the use of other pure metals or alloys was not likely to lead to more satisfactory results, the problem had to be attacked in a different way, namely, by means of an 'air' thermometer containing hydrogen under diminished pressure.

A first attempt has been made at determining the boiling-point by a constant-volume hydrogen thermometer, working under diminished pressure. This thermometer, which gave the boiling point of oxygen as 90.5° absolute or -182.5° , gave for hydrogen 21° absolute or -252° . The three determinations that have been made are then as follows: (1) pure platinum resistance thermometer, 35° absolute; (2) rhodium-platinum resistance thermometer 27° absolute; (3) hydrogen thermometer, 21° absolute. From this it appears that the boiling point of hydrogen is really lower than was anticipated, and must range between 20° and 22° absolute. Further experiments will be made with thermometers filled with hydrogen prepared from different sources. A hydrogen thermometer filled with the gas obtained from the evaporation of the liquid hydrogen itself must be employed.

The approximate density of liquid hydrogen at its boiling point was found by measuring the volume of the gas obtained by evaporating 10 cc. of the liquid, and is slightly less than 0.07, or about one-sixth that of liquid marsh-gas, which is the lightest liquid known. It is remarkable that, with so low a density, liquid hydrogen is so easily seen, has so well defined a meniscus, and can be so readily collected and manipulated

in vacuum vessels. As hydrogen occluded in palladium has a density of 0.62, it follows that it must be associated with the metal in some other state than that of liquefaction.

The atomic volume of liquid hydrogen at its boiling point is about 14.3, the atomic volumes of liquid oxygen and nitrogen being 13.7 and 16.6 respectively at their boiling points. The weight of a litre of hydrogen gas at the boiling point of the liquid is about the same as that of air, at the ordinary temperature. The ratio of the density of the hydrogen gas at the boiling point to that of the liquid is approximately 1:60, as compared with a ratio of 1:255 in the case of oxygen under similar conditions.

The specific heat of hydrogen in the gaseous state and in hydrogenized palladium is 3.4, but may very probably be 6.4 in the liquid substance. Such a liquid would be unique in its properties; but as the volume of one gramme of liquid hydrogen is about 14-15 c.c., the specific heat per unit volume must be nearly 0.5, which is about that of liquid air. It is highly probable, therefore, that the remarkable properties of liquid hydrogen predicted by theory will prove to be less astonishing when they are compared with those of liquid air, volume for volume, at corresponding temperatures.

With hydrogen as a cooling agent we shall get to from 13° to 15° of the zero of absolute temperature, and its use will open up an entirely new field of scientific inquiry. Even so great a man as James Clerk Maxwell had doubts as to the possibility of ever liquefying hydrogen.* He says: "Similar phenomena occur in all the liquefiable gases. In other gases we are able to trace the existence of attractive force at ordinary pressures, though the compression has not yet been carried so far as to show any repulsive force. In hydrogen the repulsive force seems to prevail even at ordinary

* See *Scientific Papers*, 2, 412.

pressures. This gas has never been liquefied, and it is probable that it never will be liquefied, as the attractive force is so weak." In concluding his lectures on the non-metallic elements delivered at the Royal Institution in 1852, and published the following year, Faraday said:—"There is reason to believe we should derive much information as to the intimate nature of these non-metallic elements, if we could succeed in obtaining hydrogen and nitrogen in the liquid and solid form. Many gases have been liquefied: the carbonic acid gas has been solidified, but hydrogen and nitrogen have resisted all our efforts of the kind. Hydrogen in many of its relations acts as though it were a metal; could it be obtained in a liquid or a solid condition, the doubt might be settled. This great problem, however, has yet to be solved, nor should we look with hopelessness on this solution when we reflect with wonder—and as I do almost with fear and trembling—on the powers of investigating the hidden qualities of these elements—of questioning them, making them disclose their secrets and tell their tales—given by the Almighty to man."

Faraday's expressed faith in the potentialities of experimental inquiry in 1852 has been justified forty-six years afterwards by the production of liquid hydrogen in the very laboratory in which all his epoch-making researches were executed. The 'doubt' has now been settled; hydrogen does not possess in the liquid state the characteristics of a metal. No one can predict the properties of matter near the zero of temperature. Faraday liquefied chlorine in the year 1823. Sixty years afterwards Wroblewski and Olszewski produced liquid air, and now, after a fifteen years' interval, the last of the old permanent gases, hydrogen, appears as a static liquid. Considering

* See Faraday's *Lectures on the Non-Metallic Elements*, pp. 292-3.

that the step from the liquefaction of air to that of hydrogen is relatively as great in the thermodynamic sense as that from liquid chlorine to liquid air, the fact that the former result has been achieved in one-fourth the time needed to accomplish the latter proves the greatly accelerated pace of scientific progress in our time.

The efficient cultivation of this field of research depends on combination and assistance of an exceptional kind; but in the first instance money must be available, and the members of the Royal Institution deserve my especial gratitude for their handsome donations to the conduct of this research. Unfortunately its prosecution will demand a further large expenditure. It is my duty to acknowledge that at an early stage of the inquiry the Hon. Company of Goldsmiths helped low temperature investigation by a generous donation to the Research Fund.

During the whole course of the low-temperature work, carried out at the Royal Institution, the invaluable aid of Mr. Robert Lennox has been at my disposal, and it is not too much to say that, but for his engineering skill, manipulative ability and loyal perseverance, the present successful issue might have been indefinitely delayed. My thanks are also due to Mr. J. W. Heath for valuable assistance in the conduct of the experiments.

JAMES DEWAR.

*SOME RECENT CONTRIBUTIONS TO TERRESTRIAL MAGNETISM.**

DURING the past five years a most remarkable interest in magnetic work has been shown throughout the civilized world. The present time can well be likened to the years when Gauss inaugurated a Magnetic Association, consisting of investigators from all countries, in order to carry out observa-

* A paper read before the Philosophical Society of Washington, March 17, 1900.